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Interactive Comment

Interactive comment on "Inverse transport modeling of volcanic sulfur dioxide emissions using large-scale ensemble simulations" *by* Y. Heng et al.

Anonymous Referee #1

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General comments

The manuscript presents an interesting method to estimate the SO2 emission rates from volcanic eruptions, based on model simulations and SO2 index from satellite data. While the paper presents a useful technique which are illustrated with an interesting case study of the Nabro eruption, the description of the method is not clear, in particular how satellite data is used and how uncertainties are addressed, also the results are not sufficiently validated, and references and comparisons to some other key publications on the Nabro event are lacking. The manuscript has potential for publication after being revised, with particular focus on the comments below.

Specific comments

1. “Ensemble-simulations”:

- The authors refer to the term “ensemble simulations” throughout the manuscript, which is not explained until section 3.1. It needs to be clarified in the very beginning of the paper.

- In the introduction it is stated that “The fine temporal and spatial discretization of this domain creates a need for large-scale ensemble simulations”. This is unclear. First, what is the argument for using a very finely discretized emission domain, and second, why does this create a need for ensemble?

- In section 3.1 the authors finally explain that they refer to the set of all unit simulations in the inversion procedure as an “ensemble simulation”. However, is this really characterized as ensemble simulations? Ensemble dispersion modelling implies, as explained by Galmarini et al. (2004) (doi:10.1016/j.atmosenv.2004.05.030), variations in the meteorological drivers and/or source parameters, or the approach to dispersion modelling by using different models. I do not see that these simulations fall under either of these categories. They are simply “sensitivity”-simulations, “scenario”-simulations, “unit”-simulations, or sometimes described as “source-receptor” relationships. I do not see how they are true ensemble simulations. Please justify the use of this term, or consider changing to another wording.

2. Methodology description (Section 3):

It was sometimes quite hard to follow and understand the methodology, mainly the first section 3.1. In particular I did not fully understand

- the difference or similarity between the terms “ensemble simulations” , “forward simulations” and “model forecasts”, which seem to be used interchangeably? Also, in the abstract you point out two types of simulations; “ensemble”, and “final transport simulations”. A more consistent wording is needed throughout the paper.

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- the justification for using 12 h accumulated model and observations data?

- why the importance weights need to be estimated iteratively? You say this is needed in order to obtain reliable results, please elaborate. Why do the unit simulations have to be re-run for each iteration (alas section 4.1)?

- In section 1 it is stated that the algorithm does not require explicit source-receptor relationships. But this is not clarified any further in the methodology section. Please explain.

- what is "new" compared to the methods by Stohl et al (2011) and Flemming and Innes (2013), and what are the benefits of your method compared to those previously published methods? This should be clearly stated already in the Introduction.

3. Adequate referencing:

The authors should refer to and compare their results to the following two publications which also reported on SO2-inversions and satellite-derived height estimates for the Nabro eruption:

- Theys et al. (2013) Volcanic SO2 fluxes derived from satellite data : a survey using OMI , GOME-2 , IASI and MODIS, Atmospheric Chemistry and Physics, doi: 10.5194/acp-13-5945-2013.

- Clarisse, L. et al. (2014) The 2011 Nabro eruption, a SO2 plume height analysis using IASI measurements, Atmos. Chem. Phys., 14, 3095-3111, doi:10.5194/acp-14-3095-2014

4. Altitude sensitivity:

How is averaging kernels handled in your approach (see explanations and Figure 1 of the above mentioned Theys et al. (2013) paper)? You say in section 2.2 that the AIRS SI is most sensitive to SO2 layers at about 8 to 13 km altitude which reflects the infra-red kernel. Do you take this altitude-sensitivity into account when you compare

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model and observation data? I.e. do you count the model values below 8 km in the same way as those above 8 km, or are they weighted by the averaging kernel so that low altitude model values count less since the satellite data is less sensitive at these heights? In other words, the AIRS SI data might not contain information to constrain the emissions below 3-5 km altitude. How do you deal with this?

5. Uncertainties:

How do you deal with uncertainties in the satellite SI index, and also uncertainties related to the unit simulations in particular to errors in the meteorological driving data? At longer forecast times it is likely that the errors in the meteorological data are more important.

6. Loss of SO2:

Do the MPTRAC SO2 simulations take into account decay of SO2 by for example reaction with OH? This would be important particularly on the >2 days time scales.

7. "Validation":

The authors refer in several places to validation of their algorithm by comparisons to the AIRS satellite observations. However, this is not an independent set of data since these data were used to reconstruct the altitude-dependent SO2 emission time series. This should be highlighted, and perhaps a better word is "evaluate". A better and independent dataset for validation would be IASI. In the above mentioned two papers (Theys et al (2013) and Clarisse et al (2014) there are many sources of data which you could use for validation. I consider this aspect the most important which needs extensive improvements. A thoughtful validation in lines with that presented in Theys et al. (2013) and Clarisse et al. (2014) is needed.

8. Resolution:

In Section 4.1 you specify that you use a 1 h time step and 250 m altitude step leading to 15 840 emission domains. You do not specify how much AIRS satellite data you

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have, the temporal resolution and number of SI values. Do the AIRS satellite data contain enough information to constrain the emissions at this high resolution? Would the emission subdomain at 16.25 km altitude be sufficiently different from 16.5 km altitude? This also relates to the resolution of the meteorological data (both vertically, and the 3-6 hourly temporal resolution) you used for the unit simulations. Please elaborate

9. Section 4.3:

It is not clear to me how you obtain 0.1052 kgm-1s-1 as the equal emission rate. Since you are using "binary" satellite data, i.e. the SO2 index data, how does the inversion itself produce quantitative emission rates? Or are you distributing the 1.5x10*9 kg estimate from Clarisse et al 2012 over the entire emission domain? In that case I get 1.2e9 kg / (475200 sec*30000 m) = 0.0842 kgm-1s-1. Also later you say "since the total amount of emitted SO2 is fixed" while this is not stated before. Please clarify.

10. Section 4.4:

You should highlight that the comparison of the SO2 emission rates with the aerosol observations (e.g. CALIOP) is not a direct comparison as one is gas and the other aerosol which would be either sulfate (converted from the SO2) or ash. In Clarisse et al. (2014) this is nicely explained.

Interactive comment on Geosci. Model Dev. Discuss., 8, 9103, 2015.